SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE PROVISION MADE BY MATHEMATICS FOR THE NEEDS OF SCIENCE[1]

Mathematics beyond the merest elements has been regarded by some as an excrescent malady of the human spirit, generated like the pearl in an abnormal and morbid way and representing a non-living embedment in the active tissue of the organism of society; by others it has been supposed to exhibit the highest intellectual reach of mankind, being in itself the most powerful tool yet devised for the interpretation of natural phenomena, while at the same time it affords a satisfying expression of the furthermost esthetic attainment. On the one hand, it is considered a piece of jugglery in which it is the joy of the proficient to produce more and more complicated entanglements to astonish the beholder and overwhelm him with the sense of mystery; on the other hand, it is seen to be the systematic unfolding of remarkable and important properties of a highly fascinating creation or construction of the human spirit by means of which it has at once its most intellectual delight and the best means of understanding its environment. Some workers seem to resent the interference of mathematics with their comfort in the conclusions of descriptive science and its demands that observation shall be reduced to measurable elements and the laws of nature be expressed in mathematical formulas; other thinkers believe that natural science is real science only in so far as it is mathematical, that it is only through mathematics that true science can

[1] An address delivered before the Illinois Chapter of Sigma Xi on January 17, 1917.

be understood, and that without mathematics no science can develop to maturity. One delights in observation and the record of facts, believing that he has understood a class of phenomena when he has given a general description of their relations, order and connections; the other considers the mathematical formula as "the point through which all the light gained by science must pass in order to be of use in practise."

But it is true, I believe, that mathematics is generally recognized as essential at least to scientific progress; and what I intend to do this evening is to discuss the nature of the provision made by it for the needs of science. But I can not consent to enter upon a treatment of this topic without pausing a moment, first of all, to combat a certain dangerous and apparently rather widespread error among scientists, namely, that the primary and perhaps the sole function of mathematics is to assist in the solution of the problems of science. If this were the essential purpose of mathematics, you would not find it cultivated as it is to-day. Instead of this the men who now give all their energy to its development would be allied with the particular natural sciences and would study mathematics only as auxiliary to their central concern. One could find no incentive to labor otherwise.

Fundamentally mathematics is a free science. The range of its possible topics appears to be unlimited; and the choice from these of those actually to be studied depends solely on considerations of interest and beauty. It is true that interest has often been, and is to-day as much as ever, prompted in a large measure by the problems actually arising in natural science, and to the latter mathematics owes a debt only to be paid by essential contributions to the interpretation of phenomena. But after all, the fundamental motive to its activity is in itself and must remain there if its progress is to continue.

Some mathematicians are glad when their fields of thought touch other sciences (or even practical matters); others concede the fact unwillingly or without interest; others still would perhaps consider themselves unorthodox in their feeling if they allowed any matter of connections with other things to affect at all their interest in their own fields. It was perhaps an extreme case of this feeling which prompted Sylvester to the pious or impetuous hope that no use would ever be found for the theory of invariants which he was developing with so much delight.

But it has turned out, as the mathematician is now well aware, that it is these same invariants which afford us an expression for the laws of nature. An invariant is simply a thing or a relation which remains unaltered when the elements with which it is connected undergo a certain class or group of transformations. When we know the transformations to which a class of phenomena are subject the matter of finding out the laws connecting these phenomena is a problem of invariants. It may be that a particular physical law was discovered long before the idea of invariants arose; but it is nevertheless true that a useful connection among such laws is afforded by this notion. On the other hand, we may have an equation expressing a fundamental relation among a large class of phenomena and find that this equation is invariant when the elements in it are subjected to a certain group of transformations. We may be sure that this group of transformations has something essential to do with the phenomena in consideration, and that its invariants express (partially or completely) the laws governing these phenomena. Our problem

is then to deduce these invariants and to give to them a physical interpretation.

In the theory of invariants we have an illustration of a fundamental fact concerning the applications of any but the most elementary mathematics, namely, that they arise essentially as by-products of the leading discoveries. On the part of those who make use of them they are often considered as the essential and perhaps the only results of mathematical investigation. There are some who have been impatient of those studies which apparently have no connection with less abstract considerations. But this is a short-sighted impatience. He who wishes applications and applications alone can not secure his ends better than by the encouragement of theoretical investigations even of an abstruse and remote character. Within the separate natural sciences this is so well understood that it is a matter of surprise to observe that some individuals persistently refuse to carry the conception to its logical consequence as regards the science of mathematics. But any one who has meditated much on the character of the progress of thought must certainly have a profound realization of its truth.

It seems that no body of thought has been of more importance in human progress and at the same time been criticized more freely than the science of mathematics. Much of this criticism appears to be good-natured and to amount to but little more than a quasi-humorous way of expressing the critic's own unashamed ignorance. At first sight one might treat this as harmless; but from the point of view of the general interest it can hardly be passed over in such a way. How this ignorance is to be overcome I can not say. Perhaps one of the first requisites is to find some means of overcoming the shamelessness with which individuals otherwise well trained contemplate their own ignorance of mathematics.

The mathematician himself is not disturbed so far as the welfare of his own science is concerned; but it is sometimes a matter of pain to see the general loss which arises from such ignorance and also from the severer strictures of the more pronounced adversaries of mathematics. In no other case, however, have the criticisms been so severe as those meted out to the infinitesimal calculus in the infancy of its development; and never have the fondest hopes of the founders of a science been so far surpassed by its actual achievements as here where the subject has become central in practically every field of pure and applied mathematics.

It will be instructive to examine briefly the criticisms thus met so early by the infinitesimal calculus. Some persons attacked the certainty of its principles, attempting to show that its conclusions were at variance with those obtained by methods previously known and accepted as sound. Some who labored primarily with matters of morality and religion attacked the new departure of thought on general grounds; they repulsed themselves by unwittingly displaying their ignorance of the thing which they criticized. One man, who entrenched himself in masses of calculation, pronounced the procedure of the new calculus unsatisfactory because of the indeterminacy of the form in which certain results appeared; but he afterwards acknowledged his error and admitted that he had been urged forward by malevolent persons—a thing (let us believe) which does not often happen among workers in science. Christiaan Huygens, whose opinion probably carried more weight than that of any other scientific man in his day, believed that the employment of differentials was unnecessary and declared that Leib-

nitz's second differential was meaningless. But these and many other criticisms never hindered the development of the new calculus, but served rather to aid in clearing off certain excrescences which had nothing to do with its essential characteristics and in helping it to that central place of importance which it holds to-day.

The criticism last mentioned is one which is made so often that it is profitable to dwell longer upon it. So often the mathematician hears: "What is the use of what you are doing?" He knows a thousand answers to this question; and one of the most effective is that which history has given to the criticism of the illustrious Huygens. The recently developed subject of integral equations has sometimes been confronted with the inquiry: Why develop this theory? Will not differential equations serve the purpose? But the mathematician goes calmly ahead with the development of those things which interest him just as he did formerly; and in the new case he anticipates with confidence the same triumphant justification in the event which has uniformly crowned his labors in the past.

Sometimes the criticisms directed against mathematics have grown out of a misconception of the natural limitations to which it is subject. Pages of formulas can not get a safe result from loose data. No amount of computation will remove from a result the errors already existent in the underlying observations. I have several times been confronted with the statement that mathematics made a great mistake in this or that particular case in predicting what was not found on proper examination to be true. But after all there was nothing wrong with the mathematics. It was merely that the supposed laws of phenomena on which the investigation was based were not exact. It was they and not the mathematics which were on trial.

It is true, as one of my friends said to me recently, that no machine can be constructed and completely theorized on mathematical principles alone. When this is given as a statement of fact I have nothing to say in reply. But if this natural limitation of the subject is spoken of (as I have sometimes heard) as an expression of the failure of mathematics, then an error is made which ought to be corrected. Mathematics does not claim to do the whole thing in the development of science. It simply has its rôle to perform; and there is a devoted body of workers throughout the world striving to see that it perform this function with eminent success. How it does and is to continue doing this will be apparent from the following discussion of the relation of mathematics to experimental verification.

By their very nature the conclusions of pure mathematics are not subject to experimental examination. One would not say that they are above or beyond experience, but that they are outside of it. Pure mathematics deals with certain creations of the human spirit and with these alone. So far as it is concerned, no import attaches to the inquiry after the impulse which resulted in the creation of these things. The mathematician *qua* mathematician is not interested in this matter, however much it may fascinate him as a philosopher; and he develops his science usually with perfect indifference to such considerations, rearing it from a small group of postulates or perhaps even from the general logical premises of all reasoning. In any event, he drags out into the limelight all hypotheses and keeps them vividly before him during the progress of his investigations.

In applied mathematics the state of things is very different. Here the whole treatment is bristling with implicit assumptions, some of them being carried con-

sciously while many of them are apparently unperceived. In other words, while we are applying mathematics we are at the same time making use of the customary large bundle of prejudices and preconceptions which we have not yet found a way to avoid whenever we have to treat the phenomena of nature.

In order to illustrate the way in which applied mathematics is shot through with assumptions, let us take a single case. When we begin to apply numbers in the measurement of physical quantities we assume at once that the true measure may as well be an irrational number as a rational number. For this assumption we have no *a priori* grounds or experimental reasons. We simply find it the most convenient assumption to make and we make it, having no other justification than convenience for our procedure. It may very well be true that the universe is so constructed that every measurement in it yields essentially a rational number. This would be true if all material things, all force, all energy, etc., were granular in structure and the mutual ratios of all granules were commensurable. For instance, if it should turn out that mass is to be estimated by counting the number of granules (all alike) in a body, then the mass of the body would be expressed essentially by a rational number. If electricity consists altogether of electrons of equal charge, then the measure of any charge of electricity is essentially a rational quantity. But we treat the phenomena mathematically as if essentially irrational quantities occur generally in nature. Even the continuity of space apparently rests upon just such sheer assumptions.

Starting with this fundamental hypothesis of applied mathematics, we might follow the subject from these remotely abstract regions into the things of more common thought, and in doing so we should find that such fundamental assumptions obtrude themselves at every point. If we looked persistently only at this side of the matter we should probably lose all confidence in our theoretical interpretations; but fortunately we are able always to test our results approximately with experimental data. It is not that we are testing out our processes of reasoning (we have another method for doing that); but rather that we are examining as to whether we have found a construction and interpretation which fit in with phenomena to a satisfying degree.

After these remarks I will hardly need to urge the necessity of testing in the laboratory all results obtained from given hypotheses by logical processes. For we can never know the truth of any hypothesis, or even understand its import, until we know the consequences which flow from it. Whether the conclusions reached in this be determined only by an appeal directed to experience.

Among the fields of applied mathematics that of rational mechanics has been most completely transfused by the mathematical spirit and it is here that the latter has exhibited some of its most characteristic conquests. It has here shown how high mathematical skill on the part of some investigators is necessary to the greatest progress of science, illustrating the way in which the mathematical spirit and method furnish a bond of union to the separate divisions of physical science.

So far we have considered the character of the provisions made by mathematics for the needs of science. It remains to give some specific details as to the past and some indication of apparently probable lines of development in the future. Clearly, a catalogue of specific provisions would be impossible in this address; such a thing

would require not less than a year's course of lectures. We can not hope to do more than indicate some of the central provisions.

Let us begin with a consideration of the early stages in the development of particular sciences. Each separate experimental science passes through a period of infancy in which it is not able to stand the strong meat of mathematics; and mathematical ideas initially find an application in it by slow processes. The first essential is to gather data—descriptive results, measured quantities, or what not; and only after much labor does law become apparent and the mathematical tool acquire its characteristic power.

But even in these preliminary stages mathematics makes an essential contribution in a preliminary way. It furnishes the only language in which exact information can be expressed, recorded and conveyed; and in this respect is a necessary element of that collaboration which is essential to such rapid progress as has been usual in recent years. But it does more than this. It enables us to record observations in such a way that we are able readily to grasp the relations of the various measured elements involved. I refer here particularly to the use of graphs which present data in so compact a manner and in a way so well adapted to our intuitive realization of their significance.

It would now probably be impossible to lay the foundations of any new experimental science without the collection of much numerical data, that is to say, without the use of statistics. But how are these to be interpreted? Clearly it must be by the methods of statistical mathematics.

Let us suppose now that we have made a record of our measurements of phenomena, their juxtapositions, their magnitudes, their order in time; let us assume (as we always do and must) that they are connected by law. How shall we ascertain what that law is? By what criterion shall we judge of the accuracy of our hypothetical explanation? Certainly not on any absolute grounds; we can only select the explanation which seems to us most probable. And for this our best and surest guide is and must be the mathematical theory of probability.

A science in the stage now being examined would properly be called non-mathematical, notwithstanding the preliminary use which it makes of mathematical science. Among those divisions of systematic thought which are at present in this stage of development one would probably include political science, economics, biology, psychology and geology.

At the next stage of development one would think of the individual science as having come to the period of vigorous youth but not yet mature. As preeminently the example of such a science I would select chemistry. By no means is it reduced essentially to mathematical form; and yet its laws are so stated as to be subject to the sharpest experimental verification. It employs the mathematical tools which are common in the earlier stages of science and also some additional ones. It may even employ the first derivative as a measure of rate of the reactions which it considers; but I believe that it seldom or never makes use of the second derivative.

It may be taken as a mark of the more advanced development of physics that it finds constant use for derivatives of the second order and sometimes for those of higher orders. Some one has expressed this increasing order of complexity and certain accompanying dependencies by saying that behind the artisan is the chemist, behind the chemist is the physicist, and behind the physicist is the mathematician—a pleasing climax, at least if you are a mathematician.

When we look upon physics, for instance,

as a mature science, we are not to think of it as having become dead and unproductive. Like the individual scientific worker on coming to maturity, it has merely reached the period when its deeds become most effective for the use and satisfaction of mankind. In fact, physics is perhaps at the same time the most mature natural science at present existing and the one whose recent progress has been the most rapid and the most remarkable.

In the development of physics the infinitesimal calculus has persistently played a leading rôle; its interaction with experimental results has been and is fundamental and necessary to the progress we have witnessed and yet see to-day. From this creation of the mathematicians and the use made of it by the physicists the world has received a good practically immeasurable in its extent. Sometimes we are tempted to assess the advantages due to each of these elements; but one can hardly expect success from such a venture. Logically the mathematics is prior; for it could exist of itself, while the physics probably could not. But psychologically and practically they are so bound up that no separation can be made. Were the mathematics swept away, much of physical theory would likewise have to go; but on the other hand much of the mathematics would never have existed had it not been called into being by the demands of physical science.

Until recently it was customary to assume that nature is essentially continuous in her manifestations. As long as we proceed on that hypothesis the infinitesimal calculus is the natural tool to be employed in the investigation of phenomena; and we should expect to find differential equations and integral equations playing a leading rôle in the exposition of physical theory.

That they have done so has furnished a great incentive to some investigators in prosecuting their labors. It is said that Poincaré was urged on in his studies of differential equations by the conviction that he was engaged in perfecting the most important tool which could be employed in the investigation of physical phenomena. No doubt it is a similar use of integral equations which drew quickly into that field so large a body of workers and resulted in its so rapid development. The same spur has urged men on in the study of expansion problems in connection with both differential equations and integral equations. It is now a long while since Fourier series were thus introduced; and their properties and availability have been treated in numerous investigations.

More recently extensive generalizations of these series have made their appearance; and we have a great class of expansions in the so-called orthogonal and biorthogonal functions arising in the study of differential and integral equations. In the field of differential equations the most important class of these functions was first defined in a general and explicit manner by an American mathematician, Professor Birkhoff, of Harvard University; and their leading fundamental properties were developed by him. We shall doubtless witness a great progress in our knowledge of these functions.

But in the early years of the present century the world of scientific thought has been unexpectedly confronted with a new situation of a rather astonishing sort. Our unquestioning assumption of the continuity of nature appears now not to have been well founded; and much of the development of theory which has been based on it is consequently perhaps to be regarded as only a rough and unsatisfactory first approximation. If certain apparent discontinuities in nature turn out to be real (and it looks now as if they must) then the differential equation will probably lose its place as the most important tool of applied mathematics

and the corresponding expansions will no longer serve to yield us the most satisfying form for the expression of our results.

This situation has been contemplated with uneasiness in certain quarters. To some natural scientists it has seemed like the loss of our moorings. To some mathematicians it has appeared in the light of greatly lessening the importance of many investigations, difficult and prolonged. It is said that Poincaré contemplated the outlook with keen regret. But we had as well make up our minds to the situation. It seems almost certain that electricity is done up in pellets, to which we have given the name of electrons. That heat comes in quanta also seems probable. In fact, it is not unlikely that we are on the verge of interpreting everything in nature as essentially discontinuous; and it would perhaps be no surprise to us now to find that even energy itself is not unlimitedly divisible, but exists, so to speak, in granules which can not be separated into component parts.

A few years ago such a paragraph as the foregoing would have been thought a piece of nonsense and to be not entitled to consideration; now the author is more likely to be charged with repeating something which already has been heard to the point of weariness.

In view of so sweeping and fundamental changes in our outlook, what is going to happen to the existent body of applied mathematics? Simply this, if these new ideas gain currency: that which before had been considered a very close approximation to facts will now be treated as giving only a coarse first approximation; and we shall set about the task of finding means of studying phenomena more exactly in consonance with the new underlying ideas.

You will probably be disposed to ask in what direction we shall turn now to find the requisite mathematical tools and when we can expect to have them ready for use. It may be answered that the mathematician was beforehand with a partially developed tool which will probably serve the purpose. When these new ideas in physics were just coming to the front a few young mathematicians independently of each other and apparently without knowledge of these movements in physics were engaged in the study of certain mathematical problems having to do with a thing which will probably turn out to be a suitable tool for the investigation of discrete phenomena. At any rate, the equations which they were studying are not intimately bound up with considerations of continuity as are differential equations, but yet they possess a number of properties very similar to or in common with those of differential equations. The equations which are thus brought to a new position of importance are the so-called difference equations.

Simple difference equations first appeared in the literature rather early in the history of mathematics and certain elementary aspects of their theory were considered several generations ago. But in recent years an essentially new type of problem in connection with them has come to notice; and in a short time and through several independent investigators the theory has suddenly blossomed forth into unexpected and magnificent flower.

This development had its origin almost simultaneously in three countries and in the hands of three independent investigators: Nörlund in Sweden, Galbrun in France, and myself in America. My own first contribution was followed closely (also in this country) by Birkhoff's first fundamental paper in the new field. By this time numerous other persons have made contributions to the development of the subject both from the function-theoretic point of view of the papers just mentioned

and also from another direction with a consequent consideration of a different type of problem.

So far the recent workers in this field have given their entire attention to purely mathematical developments and have not considered so much as the possibility of a use of the results in the domain of applied mathematics. In particular, my own interests have been in theoretical considerations. But I look forward to important applications of this newly developed theory both because it seems to have in it some at least of those elements which are necessary to accord with phenomena which are discontinuous in their nature and more particularly because there is here an expansion theory also consonant with the discontinuities of nature and related to difference equations in a manner somewhat similar to that in which certain other expansion problems attach themselves to differential equations. But the analogy must not be pressed too far, since there are also essential differences.

Concerning these new expansion problems I wish to say one further word. It is very recently indeed that they have come to notice; and a knowledge of them is not yet generally current. In fact, the general definition of the series involved was first made in a paper of my own published less than a year ago; and I am still engaged in working out their more detailed theory. Doubtless other workers in the near future will take up different phases of the same problem.

So far no exposition of the modern theory of difference equations exists in the literature; the results are to be found only in the original memoirs. In a few instances this theory has furnished the basis or an integral part of a course of academic lectures. As such it has appeared, as I understand, in one of the courses at Harvard. I have myself delivered lectures on it in Indiana University and the University of Chicago; and it is my purpose next year to expound this new doctrine in my lectures here. It is highly desirable that this matter shall be developed rapidly and be prosecuted from various points of view. It is in this way only that we shall be able to learn what its import really will be for the progress of science.

Before concluding my remarks I wish to speak briefly of a different sort of conception or expectation which has arisen in some quarters and having to do with a more fundamental and far-reaching use of mathematics than any yet made. It is connected with the fact that every branch of physics gives rise to an application of mathematics and the consequent feeling that there must be a deep underlying reason for this and a consequent close relation of phenomena which probably makes them capable of an explanation from a single point of view consistently maintained.

If there is a "hypothetical substructure of the universe, uniform under all the diverse phenomena," it would appear that there must be some means of ascertaining what it is and of giving to it a mathematical expression and body. At any rate the expectation of such a thing has arisen; let us hope that the event will show that the anticipation is well grounded in the nature of things.

I understand that the earliest contributions to just such a development are already in existence; that the now current theoretical accounts of radiation, diffusion, capillary action and molecular behavior in general have just such characteristics as one would expect to find in the early stages of a mathematical theory of the substructure of the universe.

Let me guard against a misapprehension concerning the foregoing remarks. I have

not been discussing the general elements in scientific progress; my purpose has been much less ambitious. In speaking to my colleagues in other fields I have tried to give an account of the faith that is in me so that they shall see what sort of motives (aside from those of esthetic delight, which however are central) the mathematician has in the work which he pursues. With this purpose before me I have spoken of just one side of the fundamental requisites of scientific progress. And now I wish to say with emphasis that I have the keenest appreciation of the use and purport of other methods and the sharpest delight in the contemplation of their achievements——achievements so different from any to be wrought out by my own familiar and loved mathematical tools. One could hardly speak of these other methods without becoming eloquent in his admiration of them. They are left unmentioned, then, not because I do not appreciate them, but because they do not fall within the scope assigned to this discussion.

My purpose will have been served if I have tended to produce in your minds a keener appreciation of the place of mathematics in the development of scientific thought; and particularly if I have induced in you a conception and feeling of the consecration with which choice mathematical spirits devote their energies to penetrating into the unknown regions of their own creations and to opening up larger areas of the enormously expanding field of mathematics which has grown more in the present generation perhaps than in any other in the history of the world.

R. D. Carmichael

SCIENTIFIC EVENTS

THE GAUTHIOT MEMORIAL

Dr. Robert Gauthiot, directeur d'études adjoint in the Ecole des Hautes Etudes, one of the most brilliant Oriental scholars of our time, died in Paris on September 11, 1916, at the age of forty, from the effects of a wound received as captain of infantry while gallantly leading his company to a grand attack. Gauthiot was a real genius, and has made lasting contributions to Iranian and Indo-European philology, playing a prominent part in the recent movement of opening up the history of Central Asia. To his ingenuity and acumen is due the complete decipherment of the Sogdian, an Iranian language preserved in ancient manuscripts which some years ago were discovered in Turkestan. He conducted two highly successful expeditions into the Pamir for linguistic exploration. Hardly had he reached the Pamir for the second time in July, 1914, when news of the outbreak of the war determined him to return to France and to take his place in the defense of his country, distinguishing himself by his bravery and receiving the croix de guerre.

The loss caused to science by his premature and tragical death is irreparable. He has left in straitened circumstances a widow and four daughters, the youngest being three years of age. A committee has been organized for the purpose of raising a Gauthiot Memorial Fund in commemoration of the great scholar, this fund to be utilized for the maintenance of his destitute family and for the publication of a Gauthiot Memorial Volume. Any further information, if desired, will be gladly given by the secretary. Contributions which will be gratefully acknowledged may be sent to some member of the American committee, or if preferred, directly by draft on Paris to Professor A. Meillet (65 rue d'Alésia, Paris XIVe, France), treasurer of the French Board of Trustees for the Gauthiot Memorial Fund.

The American committee consists of:

Martin A. Ryerson, 134 South La Salle Street, Chicago—*Honorary President.*

A. V. Williams Jackson, professor of Iranian and Sanskrit, Columbia University, New York.

James H. Breasted, professor of Egyptology and Oriental History, University of Chicago.

Walter E. Clark, professor of Sanskrit, University of Chicago.

B. Laufer, curator of anthropology, Field Museum, Chicago—*Secretary*.

AWARDS BY THE FRANKLIN INSTITUTE

THE Franklin Institute, acting through its Committee on Science and the Arts, has recently awarded medals to the authors of especially meritorious papers that appeared in the Institute's *Journal* during the year 1916. In making these awards, the committee adopted the following resolutions:

That the Howard N. Potts Medal be awarded to Professor Ulric Dahlgren for his paper entitled, "The Production of Light by Animals," appearing in various issues of the 1915 and 1916 *Journal* of The Franklin Institute, forming an original and comprehensive treatise of an extremely interesting and important subject.

That the Edward Longstreth Medal of Merit be awarded to Mr. George A. Rankin for his paper entitled "Portland Cement," appearing in the June, 1916, issue of the *Journal* of The Franklin Institute, a highly important contribution to the theory of cement chemistry.

That Edward Longstreth Medals of Merit be awarded to Professor A. E. Kennelly, Messrs. F. H. Achard and A. S. Dana, for their joint paper entitled "Experimental Researches on the Skin Effect in Steel Rails," appearing in the August, 1916, issue of the *Journal* of The Franklin Institute, containing new and valuable experimental data, heretofore unavailable to the designers of track return systems.

That the Edward Longstreth Medal of Merit be awarded to Mr. John D. Ball for his paper entitled, "Investigation of Magnetic Laws for Steel and Other Materials," appearing in the April, 1916, issue of the *Journal* of The Franklin Institute, containing new and valuable information relating to the magnetic properties of materials used in the magnetic circuits of electrical machinery.

That the Edward Longstreth Medal of Merit be awarded to Professor Dayton C. Miller, for his paper entitled "A 32-Element Harmonic Synthesizer," appearing in the January, 1916, issue, and his paper entitled, "The Henrici Harmonic Analyzer and Devices for extending and facilitating its Use," appearing in the September, 1916, issue of the *Journal* of The Franklin Institute, a comprehensive and lucid discussion of harmonic synthesis and analysis, together with descriptions of perfected apparatus for synthesizing and analyzing functions of one variable expressible by Fourier's equation.

THE AMERICAN CERAMIC SOCIETY AND MILITARY PREPAREDNESS

THE American Ceramic Society at its annual meeting held in New York, March 5 to 8, authorized the formation of a Committee on Military and Economic Preparedness, which has now been organized and has begun its activity. The committee has offered its services to the National Defense Council and the National Research Council.

This society devotes itself to the study of the chemistry and engineering of the silicate industries, embracing the manufacture of clay products, glass, cements and other cognate lines like the manufacture of abrasive wheels, the enameling of metals, etc. It does not deal with the artistic or historical phases as the name alone might lead one to infer. In its membership it has many of the leading specialists in the country, all of whom are eager to serve the country in this crisis. A census has been taken of the membership with a view to showing the number of firms and specialists available in each subdivision of the field, which have military significance.

Up to the present time eight divisions have been created which embrace in their membership leading manufacturers and technical men. The personnel of the committee is as follows:

Edward Orton, Jr., chairman, Ohio State University, Columbus, O.

A. V. Bleininger, vice-chairman, Bureau of Standards, Pittsburgh, Pa.

Divisions and chairmen of sub-committees:

Abrasives: R. C. Purdy, Norton Company, Worcester, Mass.

Chemical Stone Ware: R. H. Minton, Metuchen, N. J.

Enameled Iron and Steel: R. D. Landrum, Harshaw, Fuller & Goodwin Co., Cleveland, O.

Glass for Optical Purposes: C. H. Kerr, American Optical Co., Southbridge, Mass.

Hydraulic Cements: P. H. Bates, Bureau of Standards, Pittsburgh, Pa.

Porcelain, for Electrical Purposes, Spark Plugs, etc.: L. E. Barringer, General Electric Co., Schenectady, N. Y.

Raw Materials for the Ceramic Industries: A. S. Watts, Ohio State University, Columbus, O.

Refractories: A. V. Bleininger, Bureau of Standards, Pittsburgh, Pa.

WASHINGTON OFFICES OF THE NATIONAL RESEARCH COUNCIL

In view of the present crisis, and at the request of the Council of National Defense, the Research Council has entered into close relations with the Defense Council, acting as a department of the latter. It is, in this capacity, charged with the organization of scientific research so as most effectively to contribute to national defense directly, and to the support and development of those industries affected by the war. The original organization of the Research Council, designed primarily for peace conditions, took the form of a number of subject committees, but this has been augmented by the addition of several special problem committees, the number of which will be increased as the necessity arises.

In order to carry out this scheme of cooperation the Research Council and several of its subcommittees have secured offices in the Munsey Building, Washington, D. C., where also are the headquarters of the Defense Council. The Research Council as a whole is represented by its chairman, Dr. George E. Hale, and by Dr. R. A. Millikan, the vice-chairman, charged with the correlation of research problems in general. The work has already grown to such dimensions that Dr. C. E. Mendenhall has been granted leave from the University of Wisconsin and has come to Washington to be associated with it.

The subcommittees are represented in Washington as follows:

Military: Dr. C. D. Walcott, chairman, Dr. S. W. Stratton, secretary, and other members representing various departments of the government.
Physics: Dr. R. A. Millikan, Dr. C. E. Mendenhall.
Chemistry: Dr. Marston T. Bogert, Dr. A. A. Noyes.
Medicine and Hygiene: Dr. Victor C. Vaughan.
Engineering: Dr. W. F. Durand.

As rapidly as possible these representatives are getting into touch with defense research problems through the military branches of the government, in which matter the military committee through its secretary plays an important part, and at the same time bringing these problems to the attention of the research men and organizations. The representatives in Washington will, among other things, act as a central clearing house for the reception of problems from the government, and their proper distribution; will sift, distribute and follow up suggestions of a scientific or engineering nature received from any source, individuals or groups; and will keep those who are working on specific problems informed as to the progress being made by others working along the same lines. It is the desire of the Research Council to do anything possible to stimulate scientific activity and aid in any possible way its direction and concentration upon the most vital and immediate problems. As one further means to this end, it will shortly have available for limited distribution to investigators especially concerned, brief statements of the various problems, and some account of the conditions under which these problems develop. The attention of research men should, however, be given not only to the solution of suggested problems and the development of suggested methods, but, obviously, also to the unearthing of new problems, which may be their most valuable service.

The National Research Council may be addressed at the Munsey Building, Washington, D. C.

SCIENTIFIC NOTES AND NEWS

Dr. Arnold Hague, of the U. S. Geological Survey, distinguished for his work on the geology of the Yellowstone National Park and the Rocky Mountains, died at Washington on May 13 in his seventy-seventh year.

In the last issue of Science it should have been stated that Professor A. A. Michelson, of the University of Chicago, had been elected vice-president of the National Academy of Sciences to fill the vacancy caused by Dr. Walcott's election to the presidency.

Dr. D. T. MacDougal, director of the Desert Laboratory, Carnegie Institution of Washington, Tucson, Arizona, has been elected president for the ensuing year of the Pacific Division of the American Association for the Advancement of Science.

DR. COLIN G. FINK, in charge of the research laboratory at the Edison Lamp Works, Harrison, was elected president of the American Electrochemical Society at its recent Detroit meeting.

AT its meeting of May 9, the American Academy of Arts and Sciences on the recommendation of the Rumford Committee voted that the Rumford Premium be awarded to Professor Percy W. Bridgman, of the Jefferson Physical Laboratory, for his "Thermodynamical Researches at Extremely High Pressures."

THE Academy of Sciences at Berlin has presented the Helmholtz medal to Professor R. von Hertwig, of the University of Munich, for his embryological researches.

THE Medical Society of London has awarded the Fothergillian Medal for 1917 to Sir Leonard Rogers, of the Medical College, Calcutta, in consideration of his work on dysenteries, their differentiation and treatment.

DR. ABRAHAM JACOBI was given a dinner on May 6 on the occasion of his eighty-seventh birthday by a group of New York physicians, most of whom had been his assistants.

A DINNER was given on May 5 in the Haverford College dining-hall in honor of Dr. Henry S. Pratt, professor of biology, who has been for over six months one of the district superintendents of food distribution in northern France.

DR. MARSTON T. BOGERT, professor of organic chemistry at Columbia University, has been given leave of absence to undertake special chemical research at the request of the National Research Council.

PROFESSOR WILLIAM D. ENNIS, since 1907 head of the department of mechanical engineering in the Polytechnic Institute of Brooklyn, has been appointed major in the ordnance section, Officers' Reserve Corps.

IN ACCORDANCE with the request of the National Research Council, the faculty of Wesleyan University have appointed the following local committee, consisting of one representative of each of the scientific departments of the institution with the president, William Arnold Shanklin, *ex-officio:* Professors Walter G. Cady, physics, chairman; William North Rice, geology; Raymond Dodge, psychology; Frederick Slocum, astronomy; Leroy A. Howland, mathematics; Moses L. Crossley, chemistry.

AMONG the committees working under the general direction of the Pacific Coast Research Committee of the Pacific Division of the American Association for the Advancement of Science, of which Dr. John C. Merriam, of the University of California, is chairman, is a committee on zoological investigations on animal food supply, composed of the following: Dr. Barton Warren Evermann, director of the Museum of the California Academy of Sciences, chairman; Dr. Charles A. Kofoid, of the University of California; Dr. Wm. E. Ritter and Mr. W. C. Crandall, of the Scripps Institution for Biological Research; Professors F. M. McFarland, Jno. O. Snyder and E. C. Starks, of Stanford University, and Mr. N. B. Scofield and Dr. Harold C. Bryant, of the California Fish and Game Commission. This committee is now active in making a survey of the native animal food supply (fishes, mollusks, crustaceans, mammals, etc.) of the state, for the purpose of determining the available supply and of devising ways and means for its increase.

DR. SAMUEL E. CHIU, a recent graduate of the Western Reserve Medical School, is now director of the department of dermatology, ophthalmology and Wassermann Laboratory of the Eden Dispensary, Shanghai, China, and reports he is organizing a modern hospital in Shanghai with nursing and medical schools.

MR. F. M. ANDERSON, for many years curator of invertebrate paleontology in the California Academy of Sciences, has resigned that position in order to devote his time to special work for the Southern Pacific Company, and Dr. Roy E. Dickerson, who has been assistant curator since 1914, has been appointed curator.

MR. W. P. FRASER, plant pathologist, of Macdonald College, has been appointed to investigate the problem of grain rust on the prairie provinces of Western Canada.

Dr. O. L. Fassig, in charge of the U. S. weather bureau station at Baltimore, has gone to San Juan on a special mission to extend and reorganize the weather bureau service in the West Indies. In the Virgin Islands a station is to be established, two stations are to be started in Haiti and one at Puerto Plata, Santo Domingo. The station in San Juan will probably be designated as the station in charge of the West Indies Service.

At the annual meeting of the Boston Society of Natural History on May 2, the following officers were elected: *President,* Edward S. Morse; *Vice-presidents,* Nathaniel T. Kidder, William F. Whitney, Charles F. Batchelder; *Secretary,* Glover M. Allen; *Treasurer,* William A. Jeffries; *Councillor for one year,* George H. Parker; *Councillors for three years,* Thomas Barbour, Henry B. Bigelow, John W. Farlow, S. Prescott Fay, Robert T. Jackson, John E. Thayer, Charles W. Townsend, William P. Wharton. In addition to the annual reports of the officers, the award of the Walker Prizes in Natural History was made. The first prize of $100 was given to Alfred C. Redfield, of Cambridge, for his essay on "The Physiology of the Melanophores of the Horned Toad"; the second prize of $50 was awarded Adelbert L. Leathers, of Olivet College, for his essay on "An Ecological Study of the Chironomidæ and Orphnephilidæ, with special reference to their Feeding Habits."

At the meeting of the New York section of the American Chemical Society held in Rumford Hall on May 11, the program consisted of a symposium on "Chemical Education and Its Relation to the Profession." The speakers were Raymond F. Bacon, director, Mellon Institute, "The Professional Status of the Chemist," and Herbert R. Moody, professor, College of the City of New York, "The Call for the Chemist."

At the meeting of the Geographic Society of Chicago on May 11, Dr. Henry C. Cowles lectured on "The Trees of California, a Riddle of Forest Geography."

The foundation stone of the Carmichael Hospital for Tropical Diseases was laid a year ago. We learn from *The British Medical Journal* that during the last year the hospital has been nearly completed, the donations, amounting to £5,000, for the construction of the top story having been provided by the Calcutta firms belonging to the Bengal Chamber of Commerce. The total subscriptions to the endowment fund have risen from £20,000 to £40,000, which will allow of the completion and partial endowment of the hospital, and, in addition, annual subscriptions of over £5,000 for research, contributed by the great industries of Bengal and Assam, will be available when the school can be opened—possibly in October, 1918. Meanwhile, plans are under consideration for the addition of 80 ft., to the height of three stories, to the north wing of the laboratory. This will accommodate an out-patient department and dispensary on the ground floor, and hygiene laboratories for practical and theoretical teaching for the university diplomas in public health. A full course for this diploma has not yet been provided in India, although instruction in the prevention of tropical diseases, which are the most important from the public point of view in India, can obviously best be imparted in such laboratories as that provided in the Calcutta School of Tropical Medicine. On the third floor there will be space for further research laboratories, which will soon be required on account of the success of the endowment fund in providing several research workers in addition to the goverment staff of the school. Omitting the cost of the biological laboratory of the Medical College, which has been included in the new building for administrative convenience, the Calcutta school possesses in its laboratory, hospital and endowments, property of the value of £90,000 of which £40,000 has been provided by the government of India on the advice of Sir Pardey Lukis, Director-General of the Indian Medical Service, and an equal sum raised by the endowment fund, of which Sir Leonard Rogers is the honorary secretary. The remaining £10,000 has been found by the Bengal government, whose finances have been severely handicapped by the war. It is hoped that the Bengal government will be able to contribute some substantial help towards the hygiene ex-

tension before very long, to enable it to be opened with the rest of the building after the war. This will complete the laboratories as at present proposed, although the foundations have been designed to allow a fourth story to be added at a later date, a wide view having been taken of the future possibilities of the institution.

UNIVERSITY AND EDUCATIONAL NEWS

STANLEY COULTER HALL, the new biology building at Purdue University, erected at a cost of over $100,000, will be dedicated on May 17. This building has been named in honor of Dean Coulter in recognition of his thirty years of scientific work in the university. The dedication will be held in connection with the spring meeting of the Indiana Academy of Science at Lafayette. Professor Wm. T. Sedgwick, the Massachusetts Institute of Technology; Dr. H. C. Cowles, the University of Chicago; Dr. Carl Eigenmann, Indiana University; President W. J. Moenkhaus, of the Academy; and J. S. Wright, Esq., of the alumni, will be the chief speakers.

WESTERN UNIVERSITY receives $20,000 by the will of the late William H. Burrows, a trustee of the institution.

THE late William H. Burrows, president of the Middletown National Bank, has bequeathed $20,000 to Wesleyan University, of which he was a trustee.

BY recent action of the board of trustees of the University of Chicago, the president of the university, on recommendation of the head of a department, will welcome doctors of philosophy of the University of Chicago and other universities as guests of the university, with the privilege of attending seminars and of carrying on research in the laboratories and libraries. There will be no charge except for laboratory supplies and a nominal laboratory fee where laboratory work is done.

LELAND STANFORD JUNIOR UNIVERSITY SCHOOL OF MEDICINE has adopted the quarter system, to begin on October 1, 1917. By the adoption of this system the school has a continuous session, any three quarters constituting a college year. The quarter system has been in effect at the Rush Medical College, Chicago, since 1899.

MORRIS M. LEIGHTON, Ph.D. (Chicago, '16), has been elected to the Washington Geological Survey for next year and to an assistant professorship in geology at the University of Washington, Seattle, to take effect on September 1, 1918. Dr. Leighton substituted at the University of Washington during the year 1915–16.

PROFESSOR FREDERICK B. LOOMIS, of Amherst College, has been appointed professor of geology to succeed Professor B. K. Emerson, who is retiring from active work.

DR. WILLIAM G. MACCALLUM, professor of pathology at Columbia University, has resigned to accept the chair of pathology and bacteriology at the Johns Hopkins University and Dr. Adrian V. S. Lambert, associate professor of surgery, has been designated to serve as acting head of the department, vacant by the resignation of Dr. George E. Brewer.

DISCUSSION AND CORRESPONDENCE

WHERE DO PITCHER-LEAFED ASH TREES GROW?

AT the New Orleans meeting of the scientific societies, in 1905, I reported the discovery of a group of pitcher-leafed ash trees (*Fraxinus americana*) near the Station for Experimental Evolution, Cold Spring Harbor, Long Island.[1] These trees had one or more leaflets of nearly every leaf—especially the terminal leaflets—formed into ascidia or so-called "pitchers."

This group of pitcher-leafed trees occupies a definitely circumscribed area, surrounded on all sides by trees with only normal flat leaflets, and I supposed, until a few months ago, that the pitcher-bearing trees were limited to this single small area, and the inference seemed justified that they had originated on this area by a comparatively recent mutation.

Two new localities for this peculiar form were discovered last fall in western Pennsylvania by Professor Charles W. Palmer, of the Westtown School, Westtown, Pennsylvania, and by a friend of his to whom he explained

[1] See SCIENCE, N. S., 23: 201, February, 1906.

the situation. These discoveries indicate that the pitcher-leafed type may be an older form than I had supposed, and that it may have a rather wide distribution. As the peculiar form of the leaflets is readily observed, especially on the young trees, the fact that the occurrence of pitchers in this species has never been published except by myself, and in relation to the trees at Cold Spring Harbor, would seem to indicate that this form probably does not occur in any considerable abundance over extensive areas.

In order to work out their probable evolutionary history, it is necessary to have more complete information regarding the present distribution of these pitcher-leafed ash trees. The reader is requested to assist in securing this information during the present spring and summer, by carefully examining as many young ash trees as may be accessible to him, and reporting the result to the undersigned, giving approximately the extent of area covered by the observations, and the number of *normal* ash trees observed, as well as the number of pitcher-leafed trees—if any of the latter should be discovered. A report is just as desirable in case only normal trees are found as if pitchered specimens are found. All communications should be addressed to

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"KEEP YOUR EYE ON THE BALL"

EVIDENTLY my last letter to SCIENCE[1] was not very clear and convincing, for Mr. Patterson[2] in a recent number insists on making the inertia-reaction of an accelerated body act upon the body itself and thus oppose the accelerating force. To him the term "unbalanced force" means "*a force opposed only by inertia-reaction.*"

In avoiding confusion as to the part played by this force of reaction in any case, I have found it useful to adopt a motto from the royal game of golf—"Keep your eye on the ball." When a ball is swung on the end of a cord, the centripetal force exerted by the stretched cord *on the ball* is unbalanced and produces the centripetal acceleration of the ball. There is the whole story as far as the ball is concerned. The inertia reaction of the ball acts *upon the cord* and through the cord acts *upon the hand.* The law of motion states that the rate of change of momentum of any body is at each instant proportional to the resultant of all the forces acting upon that body. In applying the law to a given body A, keep your eye on A and consider only the forces acting upon A. Among these forces, the inertia-reaction of A is never to be included since it always acts upon some other body or bodies.

Mr. Patterson would have us believe that inertia-reaction and friction are not full-fledged forces in the single definite sense implied in the laws of motion. He says neither can produce positive acceleration. Let us see if this is true.

Consider the experiment in which two masses, connected together by a string and free to slide along a horizontal rod, are rotated about a vertical axis. If the distances from the axis are in the right ratio, the masses will rotate without sliding, the inertia-reaction of each mass accelerating the other.

Then take the case of a passenger leaning forward as he stands on a starting train. The forces acting *on him* are his weight, the upward elastic reaction of the floor which balances his weight, and friction. He is being accelerated, and friction is doing it.

Even though some forces are always pulls, others always pushes, and others neither, we need not differentiate between them since each tends to produce acceleration in a certain direction.

Let us agree, then, that a body can not exert a force upon itself; that all forces are similar in their effects; and that in applying the laws of motion to any body, care should be taken to consider only the forces acting upon it.

GORDON S. FULCHER

WISCONSIN UNIVERSITY,
March 24, 1917

1 "Can a Body Exert a Force upon Itself?" SCIENCE, Vol. XLIV., p. 747, 1916.

2 "When Is a Force Not a Force?" Andrew H. Patterson, SCIENCE, Vol. XLV., p. 259, 1917.

THE TENTH-EXPONENT

A METHOD of expressing large or small quantities in modern physical and chemical science is to write the number expressing the quantity as a factor of some power of 10. It is proposed to change this notation and write the exponent of 10 at the upper left-hand side of the factor and call this exponent *The Tenth-Exponent*. The base, 10, is omitted. The following numbers will illustrate this notation:

$1.872A \times 10^3 = {}^{3}1.872A$, number of electrons in any atom, $A =$ At. Wt.

$1.49 \times 10^{-17} = {}^{-17}1.49$ ergs, average kinetic energy of the electrons in the H atom at 0° C.

$3.4 \times 10^{10} = {}^{10}3.4$ cm./sec., *mean square* of the velocity of the electrons in the H atom at 0° C.

$4.0 \times 10^{13} = {}^{13}4.0$ cm./sec., *mean square* of the velocity of the electrons in the H atom at 3000° C.

$v/(6.062 \times 10^{23}) = v/{}^{23}6.062$ cm.,[3] the volume required by any atom,

$$v = A/D;\ D = \text{density}.$$

$6.062/v \times 10^{23} = {}^{23}6.062/v$ number of atoms of any element per cm.[3]

FRANK W. BALL

CHEMICAL PUBLICATIONS

TO THE EDITOR OF SCIENCE: On page 169 of the current volume of SCIENCE (February 16, 1917), I note that the table gives in 1914–15, 29 publications on chemistry from Columbia University, and 6 under Columbia University and Roosevelt Hospital. The work of these six papers was all done in this department, and should therefore have been included in the table. This would change the number on page 170 for Columbia University from 29 to 35, and would place this department, as regards the number of articles, eighth on the list instead of tenth. It should be noted also that this table refers only to publications in American journals. A number of papers were published in foreign journals from this department during the same year.

ALEXANDER SMITH

DEPARTMENT OF CHEMISTRY,
COLUMBIA UNIVERSITY

QUOTATIONS

SCIENTIFIC AND CLASSICAL EDUCATION

IF scientific men who have not had the time to follow up this educational controversy closely wish to grasp its essential values, they can not do better than weigh over the implications of this passage that follows, from an article by Lord Bryce in the current *Fortnightly Review:*

> I do not contend that the study of the ancients is to be imposed on all, or even on the bulk, of those who remain at school till eighteen, or on most of those who enter a university. It is generally admitted that at the universities the present system can not be maintained. Even of those who enter Oxford or Cambridge, many have not the capacity or the taste to make it worth while for them to devote much time there to Greek and Latin. The real practical problem for all our universities is this: How are we to find means by which the study, while dropped for those who will never make much of it, may be retained, and for ever securely maintained, for that percentage of our youth, be it 20 or 30 per cent. or be it more, who will draw sufficient mental nourishment and stimulus from the study to make it an effective factor in their intellectual growth and an unceasing spring of enjoyment through the rest of life? This part of our youth has an importance for the nation not to be measured by its numbers. It is on the best minds that the strength of a nation depends, and more than half of these will find their proper province in letters and history. It is by the best minds that nations win and retain leadership. No pains can be too great that are spent on developing such minds to the finest point of efficiency.
>
> We shall effect a saving if we drop that study of the ancient languages in the case of those who, after a trial, show no aptitude for them.

Let the scientific man read that over carefully, and, if need be, re-read it. Let him note first the invincible conceit of the classical scholar in the superiority of his particular education to any other, and his firm determination *to secure the pick of the available boys and the pick of the administrative posts* for the classical training. Science and research are to have those rejected as unfit in this sublime progress of the elect. Instead of our boys—I mean the boys destined for real philosophy, living literatures, science, and the study of actual social and political questions —having a straightforward, well-planned

school course, they are to be *tried over* at Greek for just the most precious years educationally, and our modern world is to have the broken fragments. This claim is pressed even more impudently by Mr. Livingstone in his recent "Defense of Classical Education." He insists that all our sons are to be muddled about with by the teachers of Greek up to at least the opening of the university stage, entirely in the interests of Greek scholarship. Professor Keeble's dream of "sweet reasonableness" is a mere dream. These classical people are absolutely ignorant of their own limitations; they can imagine no compromise; they mean to ram compulsory Greek down the throat of every able English boy they can catch, and they mean to load the scales in favor of Greek at any cost to science, philosophy and national well-being.—H. G. Wells in *Nature*.

SCIENTIFIC BOOKS

Human Physiology. By Percy G. Stiles. Philadelphia, W. B. Saunders Company, 1916. Pp. 405.

The announcement in the preface, the "purpose is to present concisely the accepted facts with only a limited description of the experiments by which these facts have been established," gives an idea of the scope and nature of the book. There is the further qualification that books of this sort are at fault if they do not make it plain that "unsettled questions confront the investigator at every turn."

Little of historical importance is mentioned, the omission being purposeful. It is an open question in the mind of the reviewer whether the student should not have some knowledge of the history of science as well as of wars and "low ambition and the pride of kings." If necessary, low ambition could be found in the history of science.

While strongly inclined to view with great charity the author's confession of a feeling akin to guilt because he has not acknowledged all the illuminating ideas and happy teaching devices which he owes to his contemporaries, the reviewer can not wholly suppress the wish that such a feeling might become highly contagious and assume a grave form among authors of text-books and the writers of papers generally. The full acknowledgment of such obligations might dim individual brilliance at times, but science would not be the loser thereby. The author's "atonement" might have been more complete if he had included the works of Ott, Stewart and Tigerstedt (English translation) in the list of collateral readings at the end of the book.

In the brief statements of a historical nature on pages 15 and 16, one finds but little mention of the influence of French investigators in physiology. A statement of Professor Howell is so pertinent in this connection that I venture to quote it.

> The establishment of physiology as an experimental science is usually attributed to Johannes Müller and his pupils or their contemporaries who fell under his influence. But as I read its history, its modern characteristics, whether for good or for evil, owe their origin as much to the French as to the German school. Johannes Müller himself was not preeminent as an experimenter—he made use of anatomical rather than physiological methods; but his contemporary Magendie was a typical modern physiologist, and whatever may have been the extent of his personal influence during life, there can be no question that his methods of work and his points of view are the ones that were subsequently adopted in physiology.

On this point, the reviewer is in full agreement with Howell. In the present world conflict of ideals and ideas, even such minor considerations as these should not be wholly lost sight of.

One departure from the usual method of treatment is found on page 95. In the legend of Fig. 22 the author states that the coordinating center for the reflex, a part of whose path is shown, is left undetermined. Evidence is accumulating that the location of the coordinating center for a reflex varies for different reflexes in the same animal, and for a reflex of essentially the same nature in different species of animals.

One might take exception to the statement on page 116 that "We are not usually aware of the nerve currents that arrive in the cen-

tral nervous system from the labyrinth." Crum Brown's statement of the function of the semicircular canals was "the perception of the change of aspect of the head in space." This statement has stood the test of criticism and one usually is aware of the change of aspect of the head in space.

The easy-going husband and the nagging wife find their counterpart in the ventricles and the auricles of the heart, p. 257. A declaration of independence on the part of either husband or ventricle leads to domestic infelicity.

On the whole, the book fulfills its particular purpose better than any other with which I am familiar. Writing such a book is a particularly difficult task, and the author has succeeded better than most. There are many new diagrams of unusual clearness.

F. H. Pike

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GOODALE'S EXPERIMENTS ON GONADECTOMY OF FOWLS

It has long been known that the removal of the testes of the male fowl does not affect materially the complete development of the secondary sexual characters of the cock, although a critical examination of the results has been much needed. The change of the hen's plumage into that of the cock, a change that occasionally takes place in old age, or when the ovary has become diseased, is also a matter of record, but the evidence for this has been rather anecdotal than detailed. Both changes have now been carefully studied by Goodale in a series of carefully planned experiments carried out through a series of years, mainly at the Station for Experimental Evolution of the Carnegie Institution. The results[1] have been published recently by the Carnegie Institution. The excellent colored drawings that illustrate the results greatly enhance their value.

[1] "Gonadectomy in Relation to Secondary Sexual Characters of Some Domestic Birds," H. D. Goodale, Carnegie Institution of Washington, 1916, No. 243.

The operation was made on Rouen ducks as well as on fowls (Brown Leghorns) and the results are in agreement in all essential respects. Complete removal of the testes either from very young, or even from older birds, does not cause any lessening of secondary sexual plumage, although in a few points the capon differs in plumage slightly from the normal cock and in a few minor points also other than plumage. The complete removal of the ovary of the birds is an extremely difficult operation and is rarely entirely successful. Failure to remove all of the tissue gives an opportunity for regeneration of the gland, which completely nullifies the attempted experiment. When removal of the ovary was complete (as shown by subsequent dissection) the duck and hen assumed the male plumage. When the very great differences in the plumage of the Brown Leghorn hen and cock and of the Rouen duck and drake are recalled, the change is startling; for it involves not only the transformation of the brown plumage of the hen into the brilliant red and black of the cock, but involves likewise a change in the shape of many of the feathers, notably those of the hackle, back, saddle and shoulder as well as minute details in the barbules. Goodale exhibited such a cock-feathered hen at the Christmas meeting of the Naturalists, as well as one case in which the testes had been removed from a young cock and an ovary engrafted in their place. The presence of the latter had prevented the cock, when adult, from developing the characteristic male plumage. He resembled a hen in essentially all plumage characters.

Into the details of the work it is not possible to enter here—details that involve the effect of incomplete gonadectomy, the possible influence of other organs in the neighborhood of the gonad, the relation between the juvenile plumage and that of the adult female, and in the case of the ducks the effect of gonadectomy on the nuptial and eclipse plumage. Several results here obtained are entirely new and a number of problems raised heretofore unsuspected.

The comb of the capon fails to reach the full development of that characteristic of the Brown Leghorn cock, while in the spayed female the comb becomes male-like in certain individuals at least. The spurs develop on the capon as well as on the cock—a result that shows that this secondary sexual character at least is little if at all affected by the removal of the testes. In the Brown Leghorn hen and more commonly in other breeds, spurs may occur on the female occasionally, and even be developed as completely as in the male, but they developed *in all* the successfully spayed females. In the light of the occasional occurrence of spur in the female, the results after spaying can not be definitely ascribed to the absence of the ovary, although this is the more probable conclusion.

In castrated drakes and in spayed ducks the voice remains normal "except that some castrated females occasionally give voice to a sound similar to the drake's." In fowls, on the other hand, both sexes after gonadectomy "are disinclined to give voice," although capons may give all the sounds characteristic of clarion (but rarely do so). The spayed females were not observed to crow.

At the time when Goodale's paper was written the effect of castration on hen-feathered males (that are characteristic of certain races, notably Sebrights, Hamburgs and Campines) was not known. Since then the reviewer has shown that not only the F_1 (dominant) and F_2 hen-feathered males assume the full plumage of the cock, but that this holds true for the pure Sebright cock also.

Goodale discusses the nature of the influence that brings about the change after removal of the ovary and concludes that the ovary secretes some substance that holds in check the development of full male characters that may be assumed to be inherited through *both* sexes. A parallel case is furnished by the experiments referred to above, in crosses of Sebrights and Black Breasted Game Bantams, that show that hen feathering is transmitted as a non-sex-linked character both by the hen and by the cock. Cock feathering develops in the hen-feathered cock after castration, as well as in the hen when old (according to a brief notice by Darwin in "Animals and Plants," Chapter XIII., Vol. 11, p. 29). The probable nature of such an internal secretion is discussed by Goodale in the following significant statement (page 49):

> The adjustment of the ovarian secretion to the characters it modifies is very close, as shown by the fact that the male characters produced in a given female are like those of the corresponding male. . . . From this we may conclude that the secretion on the whole is relatively simple and probably of uniform nature. If the secretion were composed of many substances, one to produce each effect involved, such as the change from a vermiculated feather to penciled, from a gray and white to a black and brown, the resulting complexity would be so great that one would not anticipate any such close coordination as actually results. For purposes of illustration we may assume that the ovarian secretion is simple, producing its effect by oxidation or some other simple process. The sort of result produced by oxidation, of course, depends upon the substance that is oxidized.

It need scarcely be added that this statement furnishes no grounds for the identification of the enzyme produced in the testes with the factor or factors that represent it in the sex chromosome, viz., the sex-determining factors. It is possible, of course, that the sex factors are enzymes, but there is not the slightest warrant for drawing the conclusion that they are (as some recent writers have done) from genetic evidence of this kind, for it is also possible that there may be a long series of reactions between the chemical substance in the chromosome that we may identify if it pleases us to do so as the genetic factor, and the enzyme that develops later when the testes are formed.

T. H. Morgan

Columbia University

SPECIAL ARTICLES

THE BEHAVIOR OF CERTAIN GELS USEFUL IN THE INTERPRETATION OF THE ACTION OF PLANTS

The amorphous carbohydrates constitute a very important part of the colloids of the

protoplast, the remainder of which consists largely of nitrogenous material, in the form of albumen or albumen derivatives with an unknown amount of lipin. The search for material which might simulate the imbitional behavior of growing tracts in plants begun by the senior author resulted in finding that mixtures of agar with gelatine in which the last-named substance was present in the smaller proportion showed an enhanced capacity for imbibition in distilled water and a reduced swelling in weak acid and alkali as measured in very thin plates by the auxograph.[1]

It is not certain, however, that the combination of amino-acids in gelatine is duplicated in the plant and it was deemed important to test the effects of simpler amino-acid compounds and of the more complex albumens on the swelling of agar, as representing the basically important carbohydrates. Solutions of the various mixtures were poured on glass plates in layers about a centimeter thick and 3 by 5 cm. in area. Desiccation resulted in a reduction of the length and width to about half of the original. The thickness, however, was reduced to one tenth or even as much as to one thirtieth of the original, and having a thickness of .1 mm. to .3 mm. in most cases. The principal axis of deposition of material was in the vertical and the swelling in this direction would of course be correspondingly in excess of that in the plane of the sections. It is extremely unlikely that any of the colloidal masses of the cell are iso-radial as to deposition or structure and the use of thin plates seemed a feature which might increase the similarity of behavior with that of the plant. The strands, sheets or masses of material in the cell are of course mostly thinner than the plates used in the experiments, which however, would affect speed of imbibition more than total amount.

Trios of sections of sheets of the dried colloids 2 to 4 mm. by 3 to 6 mm. were placed in the bottom of stender dishes or of heavy watch glasses securely seated on iron cylinders. Triangles of glass were placed on the sections, and the vertical arms of auxographs were rested in a socket in the center of the triangles. Any change in thickness of the sections would be registered immediately. The use of six instruments gave duplicate results of the effects of water, acid and alkali, and each record was an integration or average of the swelling of three sections.

The only albumen available when this plan was put into operation was a commercial egg-albumen, and this was first tested in mixtures with large proportions of gelatine. The results of the swellings are as follows:

Water	HCl $N/100$	NaOH $N/100$
	Gelatine (Average of 3 tests)	
313.8%	825.5%	558.3%
	Gelatine 100—Albumen 5 (Average of 5 tests)	
283.4	611.7	482.2
	Gelatine 85—Albumen 15 (Average of 5 tests)	
408.6	827.8	673.0
	Gelatine 75—Albumen 25 (Average of 3 tests)	
378.3	569.7	508.7

The albumen did not exert any important influence on the swelling of the mixture until it was present in proportions as great as 25 per cent. The action is not marked even in this high proportion. Neither this nor any other combination in which gelatine formed the greater part displayed water relations at all similar to those of the plant.

Next egg-albumen was added to agar and agar-gelatine mixtures with results as below, a further illustrative test being made of agar-gelatine:

Water	HCl $N/100$	NaOH $N/100$
	Agar 75—Gelatine 25 (Average of 4 tests)	
378.5%	427.3%	515.7%
	Agar 90—Albumen 10 (Average of 3 tests)	
1,516.6	270.0	333.3
	(Average of 6 tests)	
1,477.1	309.8	297.9
	Agar 90—Gelatine 10	
595.0	216.6	298.6

The addition of ten per cent. of albumen to agar notably reduced the capacity of agar for swelling in acid and alkali, and appeared to

[1] MacDougal, "The Imbibitional Swelling of Plants and Colloidal Mixtures," SCIENCE, N. S., Vol. XLIV., pp. 502–505, 1916.

increase the amplitude of swelling in distilled water, although the last matter is not entirely clear. The albumen reduced the swelling of a mixture containing twenty-five per cent. of gelatine slightly in acid and in alkali, but the swelling in water was not markedly greater. This preliminary test yielded results which made their extension highly desirable. Chemical analyses of the egg-albumen were not available, and as nothing was known as to the salts or other substances which might be included, it was desirable to secure material of known origin and composition. Arrangements were made with Dr. Isaac F. Harris, of Squibb and Sons Laboratory, New Brunswick, New Jersey, to prepare some albumen from beans (*Phaseolus*) and from oats (*Avena*) to be used in the mixtures. The preparations from *Phaseolus* were available in February, 1917, and the first tests were made with the "protein" extract.

Agar and gelatine were dissolved in the usual way and the temperature of the solution allowed to fall to a point below 40° C. before the protein was stirred into it. In the course of the cooling and drying cloudy masses became visible which were taken to be the globulin component of the protein. The dried sheets came down to a thickness of .3 to .4 mm. Calibrated samples were tested in trios under the auxograph in the usual manner. Two complete series of all mixtures were made and an additional measurement of the action of water and alkali were obtained. The swellings were as follows:

	Water	HCl $N/100$	NaOH $N/100$
	Gelatine 90—Protein 10 (Phaseolus)		
	585.7%	1,401.0%	942.8%
	486.0	1,200.0	704.3
	386.0	———	800.0
Averages:	485.9	1,300.5	817.7
	Gelatine 75—Protein 25 (Phaseolus)		
	696.9	818.1	621.2
	500.0	1,060.6	848.4
Averages:	598.5	939.4	734.8
	Agar 90—Protein 10 (Phaseolus)		
	800.0	50.0	150.0
	800.0	75.0	150.0
Averages:	800.0	62.5	150.0
	Agar 99—Protein 1 (Phaseolus)		
	1,080.0	300.0	220.0
	800.0	360.0	240.0
Averages:	940.0	330.0	230.0

The protein extract from the bean was thus shown to exert an influence on the swelling of agar similar to that of egg-albumen in reducing the amount of swelling in acid and alkali, and increasing it in distilled water.

The next step of importance was to ascertain the effect of some of the simpler amino-acids which might be derived from the albumens in the plant. Tyrosin and cystin were available. As an example of the method the first preparation of tyrosin was one in which one part of this substance in solution was stirred into a liquefied mass of ten parts of agar at a temperature of 32° C. This was poured on a glass slab, and as desiccation was carried out the tyrosin began to collect as a flour-like efflorescence on the surface, and apparently a large part of the substance came out in this way, so that the actual proportion of the amino-acid in the dried plate was probably not more than a fourth of the amount originally used.

The dried plate of material came down to a thickness of .15 mm. and gave the following results:

SWELLING

	Water	HCl $N/100$	NaOH $N/100$
	Agar 90—Tyrosin 10 (less by efflorescence)		
	1,600.0%	133.3%	133.3%
	1,200.0	233.3	100.0
Averages:	1,400.0	183.3	116.6

A similar preparation of agar and cystin gave the following as an average of three tests:

Water	HCl $N/100$	NaOH $N/100$
	Agar 90—Cystin 10	
2,333.3%	583.1%	328.6%

A similar mixture of agar and urea (agar 90 parts, urea 10 parts) gave the following:

Water	HCl $N/100$	NaOH $N/100$
	SWELLING	
2,173.0%	716.6%	560.2%

Urea, the amino-acids, gelatine, albumen, and the saline soluble proteins of the bean dissolved with agar and dried into thin plates show a greatly enhanced imbibition in water, an imbibition in hundredth-normal hydrochloric acid not more than a third of that in

water, while it is invariably less in alkaline than in acidified solutions. The interest in swelling which begins with a neutral desiccated section is, however, much less than that which attaches to the behavior of such material under changing conditions of alkalinity and acidity which are taken to occur in the living plant.

Dried plates of agar-protein, agar-tyrosin and agar-cystin .12 to .25 mm. in thickness and 3 by 4 or 5 mm. were placed in trios on the bottoms of stender dishes. Triangular pieces of glass were placed to cover the sections of colloid in each dish and an auxograph was arranged to give a bearing contact of the swinging arm on a socket in the center of the triangular plate. So long as the preparation remained in this condition the pen of the instrument traced a horizontal line on the sheet carried by the drum. Dried sections of the colloids have a very limited capacity for imbibition of acid and alkaline solutions, and hence it was desirable to start swelling or "growth" by an initial immersion of an hour in distilled water, which was poured in the dishes. After enlargement had begun hundredth-normal acid or alkaline solutions were used in alternation at intervals of one to three hours, as many as four changes being made in some cases before the total swelling capacity was reached. The results met all expectations based on theoretical considerations and the auxographic tracings might easily be mistaken for records of the variations of the length of a joint of *Opuntia*, for example.

Sections of plates 90 parts agar to "10" of tyrosin gave a tracing traversing 12 mm. vertically on the record paper during the first hour immersed in distilled water, remained stationary, making a horizontal line during the second hour, the water having been replaced with hundredth-normal hydrochloric acid, traversed 11 mm. of the scale in the third hour during which it was immersed in hundredth-normal sodium hydrate, then shrunk 5 mm. in an hour in acid, then enlarged 9 mm. in three and a half hours in alkali, after which it shrunk 3 mm. between 8:30 P.M. and 7 A.M. in acid. A change to alkali gave an enlargement of 6 mm. in two hours. The auxograph was set to multiply so that the actual enlargement in the periods noted was one twentieth of the distance traversed by the pen. The change from acidity to alkalinity is followed by the most marked effects when the colloid has taken up a fourth or a third of the possible total amount of water. Perhaps the most striking feature is the response of the colloid to acidification under the alternating conditions. Desiccated sections give a greater total swelling in acid than in alkali, but when a certain amount of swelling has already taken place under neutral or alkaline conditions no further increase in acid solutions takes place and actual shrinkage ensues. A change to alkalinity is always followed by increased imbibition.

The experiment in question has many features similar to those of the plant. Changes from alkalinity to acidity and the reverse must be made quickly to avoid instrumental error, consequently some acid or alkali is not removed from the dish. The plate of swelling colloid is saturated with the liquid which is being removed and neutralization, acidification or the reverse does not occur for some time. Such conditions prevail in the plant and come about even more slowly.

The disintegration of the acid of *Opuntia* beginning at daybreak does not overtake the formation of this substance until as late as 4 P.M. Whether complete neutralization or alkaline conditions ever occur in this plant is doubtful. There is ground for the assumption that it does in other plants, however.

The almost rhythmic undulations of the auxographic tracing of the elongation of a wheat leaf corroborated by measurements with the horizontal microscope suggest that growth in this organ may be accompanied by metabolic processes by which the balance of acidity and alkalinity falls now on this and then on that side, there being of course possible periods in which the growing protoplasts, or some of them, were in a neutralized state. During this time, of course, imbibition might be four to eight times as great as in either acid or alkaline conditions.

The systematic endeavor to construct a colloidal mixture which would display some of the fundamental physical properties of protoplasm of plants has resulted in finding that a mixture of substances of two of the three more important groups of constituents, carbohydrates and proteins, shows the imbibitional behavior of tissues and tracts of protoplasts of the plant. The differential action of such colloidal masses in distilled water, acid and alkaline solutions yields many striking parallels with growth. The general identity of constitution of these colloidal mixtures and of cell-masses, and the obvious similarity of their behavior, together with newly determined features of carbohydrate metabolism not described in this paper, make it possible to correlate more closely the processes of imbibition, metabolism and growth, and on the bases of their interrelation, to interpret growth enlargement and incidental variations in volume and size of organs.

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SOCIETIES AND ACADEMIES

THE AMERICAN MATHEMATICAL SOCIETY

A regular meeting of the society was held at Columbia University on Saturday, April 28. The attendance included twenty-seven members. Professor E. W. Brown presided at the morning session and Professor Edward Kasner at the afternoon session. The council announced the election of the following persons to membership in the society: Professor C. F. F. Garis, Union College; Professor F. J. Holder, University of Pittsburgh; Dr. V. H. Wells, University of Michigan; Professor W. L. Wright, Lincoln University, Pa. Six applications for membership were received.

Professor L. P. Eisenhart was reelected a member of the editorial committee of the *Transactions*. A committee consisting of Professors Focke, Cairns, Cole, Huntington, Pitcher and D. T. Wilson was appointed to have charge of the arrangements for the summer meeting of the society at Cleveland, September 4–5.

The following papers were read at this meeting:

W. B. Fite: "The relation between the zeros of a solution of a linear homogeneous differential equation and those of its derivatives."

Samuel Beatty: "The inversion of an analytic function."

Maurice Fréchet: "Relations entre les notions de limite et de distance."

O. E. Glenn: "A fundamental system of formal covariants mod 2 of the binary cubic."

Luigi Bianchi: "Concerning singular transformations B_k of surfaces applicable to quadrics."

J. E. Rowe: "The projection of a line section upon the rational plane cubic curve."

L. B. Robinson: "On partial differential equations which define certain covariants."

J. K. Whittemore: "Kinematic properties of ruled surfaces."

Olive C. Hazlett: "On Huntington's set of postulates for abstract geometry."

E. F. Simonds: "Differential invariants in the plane."

J. Douglas: "On certain two-point properties of doubly infinite families of curves on an arbitrary surface."

L. P. Eisenhart: "Conjugate planar nets with equal invariants."

Alexander Pell: "Solutions of the differential equation $dx^2 + dy^2 + dz^2 = ds^2$ and their application."

C. A. Fischer: "On bilinear and n-linear functionals."

E. B. Wilson: "Classification of real strains in hyperspace."

F. H. Safford: "Irrational transformations of the general elliptic element."

J. H. Weaver: "Some algebraic curves."

R. L. Moore: "A necessary and sufficient condition that a sequence of simple arcs of specified type should be equivalent, from the standpoint of analysis situs, to a sequence of straight segments."

Dunham Jackson: "Second note on the parametric representation of an arbitrary continuous curve."

Dunham Jackson: "Roots and singular points of semi-analytic functions."

Oswald Veblen: "Doubly oriented lines."

G. M. Green: "The intersections of a straight line and a hyperquadric."

F. W. Beal: "On a congruence of circles."

G. A. Miller: "Possible characteristic operators of a group."

R. D. Carmichael: "Examples of a remarkable class of series."

W. L. Hart: "Note on infinite systems of linear equations."

F. N. Cole,
Secretary